various ages, the corresponding line of Table I will give the information, while if the smoothed values are wanted, a similar line in the chart of isogens will give them after being smoothed, not in one dimension only *but in two dimensions.* Similarly, as regards the birth-rates for a father of any specified age and for mothers of various ages, by following the vertical columns instead of the horizontal lines.

In conclusion, I would remark that, though the method of isogens applied to Kőrösi's tables fully discusses the distribution of mean birth-rates, those tables do not enable us to determine the second postulate of paramount importance, namely, the degree of conformity of individual cases to the means of many cases. We know nothing thus far about the facility of error at the various positions in the chart, whether or no it conforms to the normal law of frequency; still less, what is its modulus, or whether the modulus is constant throughout the chart or varies in accordance with some definite law.

The answer to these questions admits of being obtained by a moderate amount of work on the original observations, selecting at first a few squares for exploratory purposes, such as are (1) distributed evenly about the chart, and (2) contain each of them not less than some 300 observations, and (3) whose means accord with the smoothed isogens that pass over the squares, thereby affording satisfactory centres of reference.

IV. "Appendix to a Communication entitled 'The Mechanical Equivalent of Heat.'"* By E. H. Griffiths, M.A. Communicated by R. T. Glazebrook, F.R.S. Received December 7, 1893.

Section I.

In a communication which I had the honour of making to the Royal Society in the spring of this year, the following statement occurs (p. 420):—"We are (with the help of Mr. Callendar) now entering on a careful direct comparison of thermometer E_m with a new form of air thermometer, which, there is every reason to believe, will give very accurate results, but we are unable to assign any definite limit to the time that this investigation may take."

A great number of comparisons have been made during the summer of this year by Mr. Callendar and myself between the mercury thermometer E_m used by me for determining the temperature of the calorimeter, the Tonnelot thermometer, No. 11,048, described in the above paper (pp. 426—433), the platinum thermometer N, by which the mercury thermometer E_m had been previously standardised,

* 'Phil. Trans.,' vol. 184 (1893), A, pp. 361—504.

and two air thermometers constructed by Hicks under Mr. Callendar's direction.*

The indications of these air thermometers are independent of external pressure, and no difficulty was experienced in obtaining the temperature of the bulb to 1/1000° C. The results were, however, not entirely satisfactory. It was found impossible to maintain the *exterior* portion of the tank (where the comparisons had to be made) at a temperature constant to 1/1000° C, especially at higher temperatures. A reference to pp. 374—378 will show that the regulating apparatus was designed to maintain at a constant temperature the *interior* of the steel chamber there described, and this purpose it fulfilled admirably. Fluctuations, however, amounting to as much as 1/100° at 50° C. occurred in the surrounding water, and an element of uncertainty was thus introduced into our comparisons. I am now so modifying the apparatus as to eliminate these fluctuations in the exterior temperature, and thus render the tank more suitable for such observations.

I believe Mr. Callendar proposes to make a communication to this Society in which the details of this comparison will be given, and he has, with this object in view, taken with him to Canada the records of our experiments. I therefore propose on this occasion to confine myself to results. I may, however, mention that extreme care was taken with the cleaning and drying of the air thermometers; observations were made with the thermometers filled with air, hydrogen, and nitrogen, and all the precautions observed which Mr. Callendar's considerable experience of air thermometers could suggest.

The conclusions, as far as they affect my previous determinations of temperature, are that over the range through which the experiments were conducted (14° to 26° C.) the limit of error does not exceed 0·003° C. of the nitrogen thermometer. An error of such a magnitude would affect my final value of J by about 1 in 4000.

Another possible cause of error, mentioned on p. 424, is the difference caused by unequal lag of the rising mercury thermometer at the beginning and end of the temperature range, and I have pointed out on p. 424 that a possible error of 0·008° C. might be due to this cause.

I have recently performed the experiments by which I hoped to throw some light on this point—using as a thermometer a naked platinum wire immersed in pentane. The experiments are difficult to conduct, and I do not regard the results as entirely satisfactory. They agree with my former experiments in indicating that the lag is greater at the beginning than at the end of the range by a quantity between 0·002° and 0·009° C. The mean result of my observations gives 0·004° C.

* For a description of these air thermometers see 'Roy. Soc. Proc.,' January, 1891.

as the difference, which would diminish our temperature range by about 1 in 3000, and would increase the value of J by a proportionate amount, although it would not affect the temperature coefficients of the capacity for heat of water or the specific heat of the calorimeter. I do not, however, feel that this point is sufficiently established to make it advisable to apply the correction to the previously published results.

Section II.

I regret to state that we have discovered a serious error in the arithmetic.

On pp. 407—410 is given an account of the comparison of the coils in the resistance-box with the B.A. standards, and Table XI gives in full the numbers actually obtained during the comparison. In order to simplify our work we constructed a table for our own use which gave the value of each coil in terms of a legal ohm, and afterwards transferred them (see p. 410) into true ohms. Unfortunately the 10-ohm coil in the bridge arm was entered in this private table as 10·0077, whereas it ought to have been 9·9977—an error of 1 in 1000, having its origin in a mistake in addition. The experimental numbers actually given in Table XI will show that the ratio of the bridge arms was 9·9734/997·87, that is, 0·0099947, or, if expressed in legal ohms, 9·9977/1000·30 instead of 10·0077/1000·30, as given on line 13, p. 410.

The mistake is obvious to anyone who compares the numbers given in Table XI with the conclusion drawn on p. 410. The whole of the arithmetic was carefully revised by both Mr. Clark and myself, but an error of this kind in simple addition is precisely the one to escape observation. The annoying circumstance is that a similar mistake in any of the other coils would have had no appreciable effect on our conclusions, but occurring as it did in the ratio of the bridge arms it affects all succeeding tables.

I would venture to add that this incident shows how advisable it is, in work of this kind, to give in full all the experimental numbers on which the conclusions are based.

In consequence of this discovery I have carefully again revised nearly the whole of the calculations, but I am glad to say that with the exception of two or three obvious misprints I am unable to detect any further arithmetical mistakes.

The effect of the correction thus rendered necessary is to decrease the value of R, in all the tables where the reduction to *true ohms* is given, by 1 in 1000; hence the value of T in Tables XXXVII, XL, XLI must be increased in the same proportion. The resulting correction is a simple one, for, as the value of J varies directly as T, it has only to be increased by 1 in 1000. Fortunately the temperatures

as obtained by the platinum thermometers are independent of the ratio of the bridge arms, and are, therefore, unaffected. The values of the temperature coefficients, of the capacity for heat of water and of the specific heat of the calorimeter, remain practically unaltered, as the correction only affects the sixth significant figure.

The corrected value of J in terms of a thermal unit at 15° C. is thus $(4{\cdot}1940 + 0{\cdot}0042) \times 10^7 = \mathbf{4{\cdot}1982 \times 10^7}$, and I estimate the limit of error due to the causes mentioned in Section I of this Appendix as $\pm 0{\cdot}0020$.

Hence (assuming that $g = 981{\cdot}17$),

J = 427·88 kilogramme-metres in latitude of Greenwich.

J = 1403·6 ft.-lbs. per thermal unit C. in latitude of Greenwich.

J = 779·77 " " F " "

V. "On the Reflection and Refraction of Light." By G. A. SCHOTT, B.A. (Cantab.), B.Sc. (Lond.), formerly Scholar of Trinity College, Cambridge. Communicated by R. T. GLAZEBROOK, M.A., F.R.S. Received November 29, 1893.

(Abstract.)

The object of this paper is to examine the consequences of supposing the transition between different refractive media to be effected continuously through a thin variable layer, to deduce expressions for the amplitudes and changes of phase of the reflected and refracted light, and to compare them with the results of experiments hitherto made on that subject.

The theories examined are the elastic solid theories, both those assuming large velocities for the pressural wave, including Green's, Voigt's, and K. Pearson's theories, and also Lord Kelvin's contractile ether theory, and then the electromagnetic theory, in the form given by Hertz, which, it may be remarked, leads to the same equations as the contractile ether theory.

The medium being continuously variable, the displacements and stresses, or the electric and magnetic force components, are everywhere continuous. The method thus avoids all hypotheses as to boundary conditions at surfaces of discontinuity.

For convenience, the first constant portion of the medium, from which the incidental light comes, is called the first medium, the second constant part, into which the light is in part refracted, is called the second medium, the thin variable part is called the variable layer, and the arbitrarily chosen planes, which include the whole of the variable layer, are called the boundaries of the layer. Since at those planes the medium is continuous, the displacements and stresses have the same values on both sides of each plane.